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WHAT PSYCHICAL RESEARCH HAS ACCOMPLISHED.

BY FRANK PODMORE, SECRETARY OF THE BRITISH SOCIETY FOR PSYCHICAL RESEARCH.

FOR centuries America has given unstinted welcome to all comers. and if her oft-abused hospitality is now extended only to the immigrant who can show his credentials, we citizens of States too old and too prudent to be so open-handed have surely no cause to complain. Seeing that with us newcomers in the world of thought plead their cause with a rope round their neck, it is matter for thankfulness that America is still willing to hear the cause first, and adjust the rope afterwards. Certainly the Society for Psychical Research can have no quarrel with Professor Minot because he has chosen to exercise his right of scrutiny in the case of that latest claimant of transatlantic hospitality—the hypothesis of thought-transference. Indeed, he has set about his self-imposed task in no ungracious spirit, it would seem. He allows us conscience, honor, honesty, and things of that description. Not all of our European critics have been willing to concede so much. He admits that the society has done good service in exposing Madame Blavatsky and the pretensions of the Theosophists, though the admission is made apparently with due sense of the difference in quality between such amateur work and a really scientific investigation. He even notes with approval the work of the society in demonstrating the fallacies and frauds of spiritualism, though he alludes only to Mr. S. J. Davey, and apparently is not aware of the work done by other members in this connection. And he has the air of being himself the discoverer of Mr. Davey, and rather quaintly insists upon drawing the attention of the Society to the lessons to be deduced from Mr. Davey's experiments—lessons which Mr. R. Hodgson, Mrs. Henry Sidgwick and others have been sedulously enforcing for some

years past. But it is at all events gratifying to find that in combating the errors, not of Theosophy only, but also of Psychical Research, our critic is compelled to choose weapons from our armory.

Then, again, Professor Minot's attitude to the whole subject is, in the abstract, irreproachable. Here in England we have been accustomed to meet sometimes with blank indifference, sometimes with active hostility. We have been told *ad nauseam*, by men who have not given half an hour to the consideration of the evidence, that thought-transference is impossible ; a compound of fraud and self-deception ; a relic of Old World superstition. Professor Minot confesses that "if the views advanced were correct, their importance could not be overestimated." He has himself, he assures us, "devoted much time to these problems," and still does not regret the time so spent. And, finally, he takes occasion to point out that to the really scientific man nothing is common or unclean. "Of the qualities which those who investigate nature must possess, almost the first is humility, . . . utter self-renunciation when in the presence of a fact."

But, leaving the abstract, let us examine his application of these large utterances to the particular case before us. This, for the moment, is the thing which Professor Minot has set himself with all humility to study : the history of the English Society for Psychical Research and the evidence for telepathy presented in its published works.

Passing over a not entirely accurate account of the origin of the society, I come to a historical statement which appears to me to involve a radical misconception of the facts. After mentioning that numerous experiments had been conducted with the children of a clergyman called Creery, in which the committee of the Society for Psychical Research claimed to have "successfully excluded deception," Mr. Minot writes : "It has since turned out that the children did have a system of signalling, which, though very simple and of a kind likely to be noticed, none of the committee did notice, so that they were not expert enough to exclude deception."

Upon this I have to remark : (1) that the signalling was detected and exposed by the committee themselves,—surely a material fact, since it proves that on this occasion, at all events, they were expert enough to exclude deception ; (2) that while

there is no evidence that signalling had been generally employed in any previous series of experiments, it is certain that, in the experiments on which reliance was placed, a code would have been useless, as the object to be guessed was known to the committee alone. This was expressly pointed out in the account of the exposure given in *Proceedings S. P. R.* (Vol. V., pp. 269-270). Special attention had also been called to the point in the account of the experiments, *published before the exposure*, in *Phantasms of the Living* (Vol. I., p. 22 *et seq.*). We find then in the statement quoted : (1) *suppressio veri*, (2) *suggestio falsi*. Now this is just the kind of misrepresentation which we should look for in a smart journalist, anxious to make out an effective case ; and, indeed, I cannot help suggesting that it is from such sources that Professor Minot possibly derived his information. A scientific student, desirous only of ascertaining the truth, may perhaps be forgiven for not realizing that persons of inferior morality—literary men and so forth—are apt to represent facts in accordance with their wishes or preconceptions. But Professor Minot can hardly be acquitted, at any rate, of some degree of negligence in not going to authoritative sources for his information.

Fortunately the proof of thought-transference is independent of the Creery experiments : and it is to be regretted that Mr. Minot's study of the subject ceased apparently five or six years ago, and that he almost wholly ignores the great mass of evidence accumulated since 1888. Moreover, his examination even of the evidence prior to that date appears to have been strangely superficial ; for in the next paragraph but one he proceeds to point out that there is a considerable probability, if one party to an experiment is told to think of a card and the other to guess the card thought of, that they will, through following an unconscious order of preference, both fix upon the same card. In this Mr. Minot is no doubt right. But he goes on to say :

“ All the English experiments of the kind fail to fulfil the conditions of scientific investigation. It was a great mistake to assume that with playing cards the chances of coincidence are 1 in 52. They are more nearly 1 in 5 or 10, but they have not been actually calculated.”

Here again I am compelled to assume that Professor Minot has had access only to imperfect sources of information, some garbled newspaper extracts, perhaps, from our *Proceedings*. Had he read

the *Proceedings* himself he must, I think, have seen that even in our earliest experiments the card was, as a rule, drawn at random from the pack, and that his criticism does not, therefore, apply. In looking through the first volume of our *Proceedings* I find on page after page such phrases as, "The card was drawn at random from a full pack," or, "Selected by cutting the pack," or, "Taken from a full pack of playing-cards." Not only is this usually stated in the detailed account of each individual series of experiments, but, in the general account of the conditions observed, which prefaced the more detailed descriptions of the committee's own experiments, it is expressly stated that this elementary precaution was taken (Proc. S. P. R., Vol. I., p. 20); and further, in order to avoid all possible misconception on so important a point, in the tabular summary of 260 trials with cards, performed under test conditions, given in the first volume of our *Proceedings* (p. 170), the reader is once more reminded in a footnote that "A full pack of cards was invariably used, from which a card was drawn at random." Finally, the precaution is enforced in our circular instructions to members (S. P. R., Vol. I., p. 297), and is referred to, I imagine, in every article or document published by us treating of experiments with cards or numbers.

In the next paragraph occurs a statement which compels us to ask whether the laws of arithmetic are indeed of universal validity, or whether Professor Minot's humility and self-renunciation have had their reward in the discovery of a new calculus. For in commenting on some experiments conducted by Mrs. Henry Sidgwick in which the number to be thought of was selected by drawing it from a bag, Mr. Minot argues as follows:

"If thought-transference is a reality, then the numbers thought of by the percipient must *not* follow the percipient's mental habit. To make a rigorous demonstration, the percipient's mental number habit should be first determined; then the agent should be given the numbers somewhat in excess, which the percipient is not likely to think of readily, and if then these numbers were reproduced by the percipient, it would indicate that there was some other factor at work than the usual mental habit."

In other words, Professor Minot believes that a "number-habit" will enable its possessor to exceed the average of correct guesses in a fortuitous series of numbers. I seem to have heard something like this theory before, but not from a professor of natural science. Those who frequent the gaming-tables, I understand, have generally a "lucky number," and card-players frequently exhibit an

unconscious preference for a particular suit. Now, to adopt a suggestion of Kant's, is Mr. Minot prepared to back his "number-habit"? Will he go to Monte Carlo, and "put his pile" on it? But I cannot seriously counsel such a course. The experiment has been tried before.

Indications of the new calculus appear again lower down:

"The English committee have sought to strengthen their case by calculating the probabilities of a given result, which they make out one in a million or trillion or more of chances. They forget that when we say a thing is improbable—meaning there is only one chance out of a large number of its occurring—we really assert that it is *certain to occur some time*."

It is easier for persons not conversant with the higher mathematics to argue upon a concrete instance. In Vol. VI. of the Proceedings S. P. R., it is recorded, as stated below, that in 644 trials with numbers of two digits the number was named correctly 117 times. Does Mr. Minot mean to say that this series of successes was certain to occur *by chance* some time or other? And that many other similar series were also certain to occur at about the same time and to the same individuals? For this is what has happened; and if his argument does not mean that, what does it mean? and what bearing has it on the facts? and, in any case, is it according to Cocker? At the end of the same paragraph occurs another statement which calls for examination. "This is not the place," writes Professor Minot, "for a lesson in mathematics, therefore it must suffice merely to allude to the fallacy of Mr. Gurney's estimate of the probabilities of thought-transference." It should be remarked, in passing, that such a lesson would have been offered not to Mr. Gurney alone, but to Professor Oliver Lodge and Mr. F. Y. Edgeworth, both of whom concur in the estimate referred to, as may be seen from their articles in the Proceedings of the S. P. R. (II., p. 257; III., p. 190; IV., p. 189. See also *Phantasms of the Living*, Vol. I., p. 26, and the quotation from Mr. Edgeworth there given, in which the odds against chance, as an explanation of a given result, are estimated in quadrillions.) These articles appear not to have come under Mr. Minot's notice; and it is perhaps again owing to some defect in the sources of his information that he speaks of Mr. Gurney's "estimate of the probabilities of thought-transference." So far as I can remember, Mr. Gurney never made such an estimate, and certainly never expressed it in figures as a million to one! Had he done

so, the offer of a lesson in mathematics would perhaps not have been inopportune. What he did do was to show that the probabilities against certain results being attributed to chance alone were to be expressed as millions or trillions to one. That is, he practically eliminated one possible cause, and left the results to be explained by one or more other possible causes, such as fraud, malobservation, misrepresentation, telepathy, and so on. This is shown very clearly in the quotations referred to from Mr. Edgeworth included in *Phantasms of the Living*, and in the following extract from one of Mr. Edgeworth's articles in the *Proceedings*:

"Such is the evidence which the calculus of probabilities affords as to the existence of an agency other than mere chance. The calculus is silent as to the nature of that agency, whether it is more likely to be vulgar illusion or extraordinary law. That is a question to be decided, not by formulæ and figures, but by philosophy and common-sense." (Proc. S.P.R., III., p. 199.)

It is to be regretted that some unfortunate chance should have robbed the Society for Psychical Research of Professor Minot's criticism on their experimental work in thought-transference. For I cannot but feel that, animated as he is by such unexceptionable sentiments, if he had afforded himself the opportunity of studying our results in an authentic form, he would have found in the work done by members of the Society in investigating telepathy the same ability and the same conscientiousness which he generously recognizes in our investigations into the fraudulent phenomena of Spiritualism and Theosophy. The investigators were the same, the methods pursued were the same, and the object was in each case the discovery of truth. Mr. Minot's unfortunate experiences are to be regretted for another reason. His energies have been so unhappily diverted to the demonstration of faults which do not exist that he has never touched at all upon the weakest spot in our experimental evidence. For whilst no fully informed critic would assert that the experiments on which we rely as establishing thought-transference are due to either chance or fraud, such as one could plausibly maintain that at least a great part of them may be explained as the result of information unconsciously conveyed by normal channels from agent to percipient, and no candid investigator would meet such a criticism with a direct negative. This indeed is the *crux* of the whole inquiry. Chance can be eliminated by a simple calculation, and fraud can be effectually

guarded against if due precautions are observed. But the unconscious operations of our organisms have been so little investigated, and are still so little understood, that a wide margin must be allowed for possible error from this cause. Perhaps an illustration will make this clearer. In the summer and autumn of 1889, Mrs. Henry Sidgwick, with the assistance of Professor Sidgwick and Mr. G. A. Smith, conducted a long and careful series of experiments in thought-transference with hypnotized subjects. The conditions were as follows: In a bag were placed eighty-one small wooden counters having the numbers from 10 to 90 stamped upon them in raised letters. From this bag Professor Sidgwick or Mr. Smith drew a counter, which was placed in a little wooden box, the edges of which effectually concealed the counter from the view of the percipient—who was, moreover, placed with his back towards the experimenters, and was in the hypnotic trance with his eyes closed. Mr. G. A. Smith then looked at the number on the counter, and the percipient would make a guess at it. The guesses and *all the remarks made* were recorded at the time by Mrs. Sidgwick, who was, during the greater part of the series, in ignorance of the number drawn. There was, of course, no contact between agent and percipient. The following is a verbatim account of a series of trials made on July 6, 1889, the percipient being a youth named T—. Mr. Smith is indicated in the record by the letter S. :

T.'s eyes were apparently closed, and he kept his head very still, and we ascertained by experiment that he would have had to move it several feet to see the number. The impression sometimes came to him quickly, and sometimes slowly—as the remarks recorded show. He was only told that he was to see numbers of two figures.

Number Drawn.	Number Guessed and Remarks.
61	... T.: "26."
84 T.: "A 3 and a 2, I believe—32."
47	... T.: "Is it 07—02; it can't be that?"
32 T.: "Looks like 1—can't see the first figure—I think it's a 6—61."
80 T.: "11, isn't it? two ones." S.: "Have a good look." T.: "11."
21 T.: "Seems like 2: 25, is it?"
18 T.: "I believe it is 1 and 0."
56 T.: "Can't see anything." S.: "You'll see it in a minute." T.: "There's a 6, and, I believe, a 2—26, I think."

59 T.: "No" (meaning that he saw nothing). S.: "You'll see it in a minute." T.: "No, can't see it . . . Believe it is 14."

37 T.: "I see a 3; there are three of them—147."

61 T.: "That's 61, I think."

33 T.: "No, I can't see—can't see that." S.: "Wait a minute. (Pause.) "Do you see them now?" T.: "No, I can't." (A long pause.) T.: "A funny thing that is—a mixture, 5, 8—looks like a 3 or an 8—3, I think."

40 T.: "4, 0, I think."

21 T.: "Is it 2?" S.: "Well?" T.: "2, 3, I think." S.: "Sure about the 3?" T.: "Yes." (After a pause, the number having been meanwhile put back in the bag.) "Oh, yes," as if he got surer and surer.

47 T.: "Is it a 5? 5 and 8."

60 T.: "6, that's all." S.: "Are you sure there's nothing more?" T.: Oh, yes, 61."

74 T.: "Is it a 4?" "There's a 4 and a 7. No, it's not. Oh, dear, no, it's 5, I think—54."

22 T.: "It's 20." (Pause, obviously trying after the second digit.) "22."

38 T.: "It's 5 and . . . 35."

45 T.: "I see nothing at all." (Pause.) "No, I can't see it. What makes it so long in coming? Now I can see it. It's a 4 and 5."

59 T.: "What makes them so long coming? I see something like a 2. It's a 2. Oh, it's a 9; I think 29." S.: "Are you sure about the first one?" T.: "Yes; 29."

66 T.: "Oh, yes; it's *two sixes*."

21 T.: "Oh, it's a 1 and a 2, 21. Ain't there a lot of them!"

83 T.: "Is it a 3?" S.: "Well, what else?" T.: "Nothing else."

80 T.: "It's 80." S.: "That's right."

73 T.: "— Such a lot of numbers as this!"
(T. spoke very low and drowsily, and Mrs. Sidgwick failed to catch the beginning of this sentence.) S.: "Yes, when we're looking for them." (Pause.) S.: "What are you looking at?" T.: "Nothing." S.: "I thought you said you saw a lot of figures?" T.: "A 3 to the right. I believe there's an 8." S.: "Are you sure?" T.: "Yes; 693." (S. said there were only two figures.) S.: "You must have seen the 6 twice over, once reversed as 9." (Possibly the idea of three figures was due to Mr. Smith's remark about a lot of figures.)

83 "85."

21 "24."

Not noted. T.: "3, I think—83." S.: "Sure?" T.: "Oh, no, it's reversed according to 38."

our recollection after-
wards the
guess was
partly right.

Possibly the idea of its being reversed may have arisen from Mr. Smith's remark above about 6 being seen reversed as 9—a remark which had puzzled T. at the time. We asked T. how the numbers looked when he saw them. He said "They're a kind of white—grayish white." He had not seen the numbers used in his waking state.*

33 T.: "A 6 and a 4." (After a pause.) "95."

78 T.: "38." S.: "Sure?" T.: "Yes."†

It will be seen that, leaving out of account the instances in which one or other digit was named correctly, the subject named the whole number correctly 7 times out of 31 trials—the most probable number of correct guesses being 1 in 81. In the whole series of these experiments 644 trials were made, the number being correctly named (*i. e.*, with both digits in their proper order) 117 times, and with digits reversed 14 times.

It is clear that chance cannot explain these results. It is almost equally clear that they cannot be attributed to fraud, unless, indeed, we suppose that not one, but all the experimenters, were in collusion. There still remains the possibility that the information was given unconsciously, and, probably, received unconsciously. Apparently, under the conditions described, the only normal channel of communication would be by the ear. It may be suggested, for instance, that Mr. Smith muttered the word audibly to the percipient. This hypothesis must, indeed, be regarded as extremely improbable, for various reasons: (1) Mr. Smith himself and the other experimenters were fully aware of this danger and on their guard against it. (2) No movements of Mr. Smith's lips were observed by the two trained and vigilant witnesses. (3) An analysis of the failures does not show that there was any tendency to mistake one number for another similar in sound.

Nevertheless it may be admitted, especially in view of the possible hyperesthesia of hypnotized subjects, that if these experiments stood alone the hypothesis that the information was actually conveyed by auditory means might be preferable to the hypothesis of a new mode of communication. But they do not stand alone. They are but one of many groups of experiments conducted by different observers and under varying conditions, and no one hypothesis will cover them all. Mrs. Henry Sidgwick again, assisted by Mr. Smith, Miss Alice Johnson and others, conducted a further

* As a matter of fact, the numbers were stamped in red on a plain wood surface.

† Proc. S. P. R. Vol. VI., pp. 132-134.

series of experiments in which the agent and percipient were in different rooms. In some of these experiments the agent and percipient were on different storeys of the house, separated by a wooden floor covered with a thick Axminster carpet. In others the percipient was in a room with the door closed and the agent, Mr. Smith, was outside in the passage, the distance between them varying from 10 to 15 feet and upwards. Both agent and percipient were under close observation throughout the trials; and it seems incredible that any sounds which escaped the notice of the observer who sat close to the agent and watched him continuously could have been perceptible to the percipient sitting at a considerable distance, and with a closed door or a ceiling and carpet intervening. In these experiments out of 252 trials, the number was guessed correctly 27 times, and with digits reversed 8 times (Proc. S. P. R., Vol. VIII., pp. 536-596). If further evidence is needed it will be found in the experiments on the production of sleep at a distance carried on by Professor Pierre Janet and Dr. Gibert, Dr. Hericourt, and other French observers; and in the production of other effects at a distance by M. Roux, Mr. Joseph Kirk, Dr. Gibotteau and others. Men who have really examined the subject will, I think, admit that the evidence accumulated during the last ten years, if it does not demonstrate the reality of thought-transference, at least establishes its claim to our consideration.

I now pass on to consider briefly Professor Minot's comments on another part of the evidence put forward by the Society for Psychical Research. Believing telepathy to be a *vera causa*, we have sought to show that apparitions coinciding with the death or sudden illness, etc., of the person represented can plausibly be explained as hallucinations evoked by the action of one mind upon another at a distance. Our line of proof is twofold. In the first place we have obtained evidence that hallucinations of this kind have on several occasions been produced experimentally in some person at a distance by the mere will of the agent. In the second place we have endeavored to show that the coincidences between apparitions and the death of the person represented (to take the most crucial instance) are too numerous to be explained by chance. Professor Minot's comments are marked by the same curious ignorance of what we have really done and written in connection with this subject. He deals only with the

second line of argument, and tells us that if the subject is to be examined scientifically it should be viewed from the standpoint either of psychology or ethnology. "The psychologist," he says, "would at once tabulate the results, determine the proportion of phantasms seen by men and women, their relation to the age of the percipient, the time of day, etc. He would have sought for every possible factor in the percipient's condition." He then proceeds to say that he suggested this line of inquiry to Mr. Gurney, who replied "to the effect that this was not worth while." The accuracy of Mr. Minot's recollection of what Mr. Gurney said to him can perhaps best be gauged by what Mr. Gurney actually did in this connection. He devoted many laborious months of his life and some hundreds of pages of *Phantasms of the Living* to the study of hallucinations in general, their relation to the particular species of telepathic hallucinations, and to the tabulation of the result of an inquiry, continuing over several years, into the distribution of hallucinations, whether coincidental or not, amongst sane persons. Amongst other points dealt with in this inquiry are the proportion of hallucinations amongst men and women, their relation to the time of day, the health and occupation of the percipient, etc., etc. Once more I am bound to assume that Mr. Minot has not read the book, *Phantasms of the Living*, to which he refers in his article. It should be added that the inquiry which Mr. Gurney thus began has during the last four years been carried out on a much larger scale at the request of the International Congress of Experimental Psychology which met at Paris in 1889. The report of the committee charged with this investigation, of which Professor Sidgwick was the head, was read at the meeting of the Congress held in London in 1892, and has recently been published in full in the Proceedings of the S. P. R. (Vol. X.). This report is based upon the results furnished by questioning no less than 17,000 persons as to whether they had experienced sensory hallucinations; nearly 10 per cent. of the answers being in the affirmative. It deals with the nature, mode of occurrence and development of hallucinations in health, and their distribution according to age, sex, physical condition of the percipient, sense affected, and so on. It will be seen that neither at the time of which Professor Minot apparently speaks, nor since, has the Society neglected the scientific study of hallucinations.

As against his assertion that the narratives of this class which

we have published "emphatically suggest myth-stories," I will place an equally emphatic statement on the other side by Professor Royce (whom Mr. Minot quotes with approval). In a report on "Phantasms and Presentiments" presented to the American Society for Psychical Research in 1889, Professor Royce writes: "Our stories bear in general the marks of being not mere products of folklore or of systematic superstition, but rather expressions of genuine experience—of experience which our correspondents do indeed often misinterpret, but which are in most cases the fresh, live product of real mental processes, and *not* the manufactured tale of popular legend" (Proc. American S. P. R., p. 350).

At the outset of the work Mr. Gurney was confronted by an argument seemingly of some weight, which he states as follows: "All manner of false beliefs have in their day been able to muster a considerable amount of evidence in their support, much of which was certainly not consciously fraudulent." What right had we then to assume that our evidence was more trustworthy than that by which these admittedly false beliefs were supported? In order to test the validity of this argument Mr. Gurney decided to examine the grounds for the belief in witchcraft. He chose this partly because it was the classic instance of a false belief alleged to have been amply supported by evidence; partly because there is an immense and comparatively recent literature dealing with the subject, most of it written by men of eminence in their day. Mr. Gurney succeeded in showing that there is practically no first-hand evidence for the more extreme marvels, except that of occasional drunkards, or confessions extracted under torture; and that the only phenomena fully attested by eyewitnesses were such as could readily be explained by hallucination, hysteria, trance and other known causes. I cannot think that Mr. Minot's account of the matter is adequate. **Mr. Gurney**

"has studied through the literature of lycanthropy and like superstitions, and finds naturally no first-hand accounts. That conclusion is interesting and corrects the false notion that there was more or less of such evidence, but it has little bearing on the question in hand. If Mr. Gurney had shown that there was no evidence of magic exciting effect on persons at a distance, or that there was no direct evidence of an astrological horoscope being fulfilled, he would have had some defence for his position."

If magic and astrology had had as wide a vogue as witchcraft;

had formed the subject of recent and serious discussion by men of learning and repute, and had left so ample and accessible a literature, no doubt it would have been worth while to examine their claims as well. But Mr. Minot has failed to make clear why such studies should have been preferred to the study of witchcraft, unless, indeed, because Mr. Gurney did not explicitly deal with them, and did deal with witchcraft. As it is, Mr. Gurney achieved his purpose by correcting the "false notion" referred to, that the widespread belief in witchcraft was based upon direct evidence.

In conclusion, I must demur to Professor Minot's statement that "the leaders of the Psychical Society are literary men"—if that means that their chief claim to distinction is their literary work. Amongst those who are taking, and have taken from the first, a prominent part in the work of the Society in England are Professor Henry Sidgwick, Professor W. F. Barrett, and Professor Oliver Lodge, to whom I may add the late Professor Balfour Stewart. Abroad, among other names that occur to me, are those of Charles Richet, of Paris, and William James, of Harvard. I do not think that any one who is acquainted with the history of science at large during the last few years would consider that any one of those gentlemen was adequately described as a literary man. And I must again demur to the lesson which he draws from his unfounded assumption. To my thinking a man should be judged by his work; and even a literary man, without any scientific pretensions, when he has taken the trouble to study with candor and care the subject on which he writes, may on occasion be worth the hearing.

It would not become me to deliver a panegyric on the men with whom for many years I have had the honor of co-operating. But this much I may say for myself and my colleagues. We have never consciously misrepresented a fact or done less than justice to an opponent; in the slow and cautious pursuit of a novel inquiry, hampered by untried material and unfamiliar conditions, we have learned much from our own mistakes and shortcomings, and as much as we could from the comments of our critics.

This inquiry into telepathy has now extended over more than twelve years, and has cost much hard thought, prolonged research, patient and laborious accumulation of observation and experiment. The records of its results, alas! fill many thousands

of printed pages. We ask only for a fair hearing. The cheap methods of the journalist are out of place here. Even the *obiter dicta* of a scientific man, however otherwise eminent, do not meet the requirements of the case ; and to criticise our methods and the results of the inquiry after a superficial reading of our earlier publications, leaving out of sight the work done after experience had been gained and methods of investigation perfected by trial, seems unfair, not more to the men whose work is thus misrepresented, than to the public, who look to the critic for guidance.

Such work can be judged only as a whole, and only by men who are willing to devote to the study of the results some small fraction of the pains which went to the accumulating and recording of them.

FRANK PODMORE.